

XV. *Experiments on the Production of Dephlogisticated Air from Water with various Substances. In a Letter from Sir Benjamin Thompson, Knt. F. R. S to Sir Joseph Banks, Bart. P. R. S.*

Read Feb. 15, 1787.

DEAR SIR,

Munich, Sept. 1, 1786.

VARIOUS opinions having been entertained with respect to the origin of the dephlogisticated air, produced by exposing healthy vegetables in water to the action of the sun's rays, according to the method of Dr. INGEN-HOUZ; and not being myself thoroughly satisfied with any of the theories proposed, I made the following experiments, with a view to throwing some new light upon that subject.

Having found that raw silk possesses a power of attracting and separating air from water in great abundance, when exposed in it to the action of light, it occurred to me to examine the properties of this air, and to consider more attentively the circumstances attending its production, thinking that this might possibly lead to some further discoveries, relative to the production of the air yielded by water under other circumstances: and though my success in these inquiries has not been equal to my wishes, yet, as in the course of my researches I have discovered some facts which I take to be new, and as I have confirmed others, already known, by a variety of new experiments,

ments, I flatter myself that you will not think an account of my labours upon this subject altogether uninteresting.

Before I enter upon the detail of my experiments it will be necessary to premise, that I shall in general confine myself merely to the facts as they appear, without applying them to the confirmation or refutation of the theories of others, and without entering into any speculative enquiries relative to their remote causes; and in describing the different appearances I shall make use of the most familiar terms. Thus, in speaking of the air produced upon exposing raw silk in water to the action of light, I shall sometimes mention it as being yielded by the silk; and I shall sometimes speak of the air furnished by exposing water, which has previously turned green, in the sun's rays, as being immediately produced by the water, though it is probable, that the *green matter* acts a very important part in the production of this air in the one case and in the other. But how it acts is not well ascertained; and I had in general much rather confine myself to a simple, and even an unlearned, description of facts, than by endeavouring to give more precise definitions, at first, to involve myself in all the difficulties which would attend accounting for phænomena, whose causes are but very imperfectly known.

You will, therefore, not be surpris'd, if you should sometimes find me speaking of appearances in the same manner as a person would mention them who saw them for the first time, and who did not know that others had discovered them before, and how they had endeavoured to account for them. I shall take care that the facts shall be faithfully described, and I flatter myself you will not think them the less interesting on account of their being unadorned.—But I hasten to give you an account of my experiments.

Experiment N° 1.

My first object was to collect a sufficient quantity of the air separated from water by silk to determine its goodness by the test of nitrous air; and to this end, having filled with clear spring water a globe of thin, white, and very transparent glass; $4\frac{1}{2}$ inches in diameter, with a cylindrical neck $\frac{3}{4}$ of an inch in diameter, and about 12 inches long, I introduced into it 30 grains of raw silk, which had been previously washed in water, in order to free it of air; and inverting the globe under water, and placing its neck in a glass jar, containing a quantity of the same water with which the globe was filled, I exposed it in my window to the action of the sun's rays, and prepared myself to examine the progress of the generation or production of the air.

The globe had not been exposed ten minutes to the action of the sun's rays, when I discovered an infinite number of exceedingly small air-bubbles, which began to make their appearance upon the surface of the silk; and these bubbles continuing to increase in number, and in size, at the end of about two hours the silk, appearing to be intirely covered with them, rose to the upper part of the globe.

These bubbles going on to increase in size, and running into each other, at length began to detach themselves from the silk, and to form a collection of air at the upper part of the globe; but the measure of my eudiometer being rather large, it was not till after the globe had been exposed in the sun near four days, that a sufficient quantity of air was collected to make the experiment with nitrous air, in order to ascertain its goodness by that test.

Having at length collected a sufficient quantity of this air for that purpose, I carefully removed it from the globe, and mixing with 1 measure of it 3 measures of nitrous air, they were reduced to 1,24 measures; which shews, that it was actually *dephlogisticated air*, and that of a considerable degree of purity.

Common air, tried at the same time, 1 measure of it with 1 measure of nitrous air were reduced to 1,08 measure.

Having again exposed the globe with the same water and silk in my window, where the sun shone the greatest part of the day, at the end of three days I had collected $3\frac{1}{4}$ cubic inches of air, which, proved with nitrous air, gave $1a + 3n = 1,18$; that is to say, 1 measure of this air, added to 3 measures of nitrous air, were reduced to 1,18 measure.

A small wax taper, which had been just blown out, a small part only of the wick remaining *red-hot*, upon being plunged into a phial filled with this air, immediately took fire, and burnt with a very bright and enlarged flame.

The water in the globe appeared to have lost something of its transparency, and had changed its colour to a very faint greenish cast, having at the same time acquired the odour or fragrance proper to raw silk.

This experiment I repeated several times with fresh water (retaining the same silk) and always with nearly the same result; with this difference, however, that when the sun shone very bright, the quantity of air produced was not only greater, but its quality likewise was much superior to that yielded when the sun's rays were more feeble, or when they were frequently intercepted by flying clouds. The air, however, was always not only much better than common air, but better than the air in general produced by the fresh leaves of plants exposed in water to the sun's rays in the experiments of Dr. INGEN-

HOUSZ; and under the circumstances the most favourable, it was so good that 1 measure of it required 4 measures of nitrous air to saturate it, and 3,65 measures of the two airs were destroyed; or, proved with nitrous air it gave $1a + 4n = 1,35$, which, I believe, is better than any air that has yet been produced in the experiments with vegetables.

The method I have here adopted of using algebraic characters in noting the result of the experiments made to determine the goodness of air, though not strictly mathematical, is very convenient; and for that reason, I shall continue to make use of it. a represents the air which is proved; n nitrous air; and the numbers which are joined to these letters shew the quantities, or the number of measures, of the different airs made use of in the experiment. The other number, which stands alone, or without any letter attached to it, on the other side of the equation, shows the volume, or the number of measures and parts of a measure to which the two airs are reduced after they are mixed. I shall sometimes add a fourth number, shewing the quantity of the two airs destroyed, as this more immediately shews the goodness of the air which is proved.

Thus, in the experiment last mentioned, 1 measure of the air proved, mixed with 4 measures of nitrous air, were reduced to 1,35 measure, consequently 3,65 measures of the two airs were destroyed; for it is $1 + 4 = 5 - 1,35 = 3,65$, and the result of this trial I should write thus, $1a + 4n = 1,35$, or 3,65.

Or, for still greater convenience in practice, as this last number 3,65, or $3\frac{65}{100}$ shews more immediately the goodness of the air in question, as I have just observed, by supposing with Dr. INGEN-HOUSZ the measure of the eudiometer to be divided into 100 equal parts, it will be $100a + 400n = 135$, and 365, expressing

expressing the volume of the two airs destroyed, will become a whole number.

But, instead of writing $100a + 400n = 135$, &c. I shall continue to write $1a + 4n = 1,35$, and shall express the last number (3,65) as a whole number notwithstanding; and I shall sometimes (following the example of Dr. INGEN-HOUSZ) write this number *only*, in noting the goodness of any air in question.

I would just observe, with respect to the process of proving the goodness of any kind of air, by the test of nitrous air, that I mix the two airs in a phial, about 1 inch in diameter and 4 inches long, putting the air to be proved into the phial first, and then introducing the nitrous air, one measure after another, till the volume of the two airs after the diminution has taken place, amounts to more than *one* measure, and is less than *two* measures.

Immediately after the introduction of each measure of nitrous air, I give the phial a couple of shakes; after which I suffer it to stand at rest, while I prepare another measure of nitrous air, which commonly takes up about 20 seconds.

The measure of the eudiometer being filled with air, I suffer it to remain quiet under water 15 seconds, or while I can leisurely count 30, in order that the air may have time to acquire the temperature of the water in the trough, and that the water in the measure may have time to run down from the sides of the glass tube; and in shutting the slider I take care to bring it to be exactly even with the surface of the water in the trough. Similar precautions are likewise made use of in measuring the volume of the two airs in the tube of the eudiometer, after they have been mixed and diminished in the phial.

In order that I may know when I have added nitrous air enough to the air in the phial, so that the volume of the two

airs may amount to 1 measure, and may not be greater than 2 measures, there are two marks upon the phial, made with the point of a diamond, the one shewing 1 measure of my eudiometer, the other shewing 2 measures.

The tube of my eudiometer is half an inch in diameter internally, and 1 measure occupies $3\frac{1}{4}$ inches in length upon it, and the measure itself is made of a piece of the same tube. Both the one and the other are ground with fine emery on the inside, in order to take off the polish of the glass, and by that means facilitate the running down of the water, which might otherwise hang in drops upon the inside of the tube upon the introduction of air.

The nitrous air was always fresh made, and of the same materials, *viz.* fine copper wire dissolved in smoking spirits of nitre, diluted with 5 times its volume of water; and all possible attention was paid to every other circumstance that could contribute to the accuracy of the experiments.

I have thought it necessary to mention these particulars on account of the great difference in the apparent goodness of any kind of air proved by the test of nitrous air, which arises from the difference of the circumstances under which the experiments are made.

But to return to my experiments upon the air produced upon exposing silk in water to the action of the sun's rays.

Experiment N^o 2.

Finding that the quantity and the quality of the air produced depended, in a great measure, upon the intensity of the light by which the water and the silk were illuminated, I was desirous of seeing whether by depriving them intirely of all light, they would not at the same time be deprived of the power of
furnishing

furnishing air. To ascertain this fact, I took a globe A, similar to that made use of in the foregoing experiment, and having filled it with fresh spring water, I introduced into it 30 grains of raw silk, and placing it with its cylindrical neck inverted in a jar filled with the same water, I covered the whole with a large inverted earthen vessel, and exposed it, so covered up, for several days in my window, by the side of another globe B, containing a like quantity of water and silk, which I left naked, and consequently exposed to the direct rays of the sun.

The result of this experiment was, that the water and silk in the globe exposed to the sun's rays furnished air in great abundance, as in the experiment before-mentioned; while that in the globe covered up in darkness, produced only a few very inconsiderable air-bubbles, which remained attached to the silk.

Experiment N^o 3.

To see if heat would not facilitate the production of air in the globe sheltered from the light, I now removed it from the window, and placed it near a German stove, where I kept it warmed to about 90° of FAHRENHEIT's thermometer for more than 24 hours; but this was all to no purpose. The air produced was so exceedingly small in quantity, that it could neither be proved, nor measured, there being only a few detached air-bubbles, which had collected themselves near the top of the globe.

The medium heat of the water in the globe exposed in the sun's rays, at the time when it furnished air in the greatest abundance, was about 90° FAHRENHEIT. It was sometimes as high as 96°; but air was frequently produced in considerable quantities when the heat did not exceed 65° and 70°.

Experiment N° 4.

Finding by the last experiment (N° 3.) that heat alone, without light, was not sufficient to enable silk in water to produce air, I was desirous of seeing the effect of light, without heat, upon them. To this end, I took the globe B, with its contents, and plunging it into a mixture of ice and water brought it to the temperature of about 50° F. and taking it out of this mixture, and exposing it immediately in the sun's rays (which were very piercing at the time) I entertained it in this temperature above two hours by the occasional application of cloths, wet in ice water, to the lower part of the globe.

Notwithstanding this degree of cold, a considerable quantity of air was produced; though it was not furnished in so great abundance as when the globe was suffered to become hot in the sun's rays.

Having thus ascertained the great effect of the sun's rays in the production of the air furnished upon exposing silk in water to their influence, my next attempt was to determine, whether this arose from any peculiar quality in the sun's light; or whether *other light* would not produce the same effect. With a view to ascertaining this point, which I conceived to be of very great importance, I made the following interesting experiment.

Experiment N° 5.

Having removed all the air from the globe B, and having supplied its place with a quantity of fresh water, so as to render it quite full, I replaced it inverted in its jar, and removing it into a dark room, surrounded it with 6 lamps with reflectors,
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and 6 wax-candles, placed at different distances from 3 to 6 inches from it, and so disposed as to throw the greatest quantity of light possible upon the silk in the water, taking care at the same time that the water should not acquire a greater heat than that of about 90° F.

Things had not remained in this situation above 10 minutes, when I plainly discovered the air-bubbles beginning to make their appearance upon the surface of the silk; and at the end of 6 hours, there was collected at the upper part of the globe a quantity of air sufficient to make a proof of its goodness with nitrous air; and, upon trial, I had the pleasure to find, that it was *dephlogisticated*, and of such a degree of purity, that 1 measure of it with 3 measures of nitrous air occupied no more than 1,68 measure.

I afterwards exposed, to the same light, in small inverted glass jars, filled with water, a fresh-gathered healthy leaf of the peach tree, and a stem of the pea plant with 3 leaves upon it; and both these vegetables furnished air in the same manner as they are known to furnish it when exposed, under similar circumstances, to the action of the sun's direct rays, but in less quantities, which I attribute to the greater intensity of the sun's light above that of my lamps.

The experiment with the silk and water I repeated several times, always with nearly the same result. The quantity of air furnished was sometimes a little greater, and sometimes a little less; but it was always in much greater abundance than that furnished by an equal quantity of water and silk exposed to the same heat, but excluded from the light; and I have reason to think, it was of a much superior quality, though the quantity of that produced in the dark was too small to be submitted to any proof.

These experiments appear to me to be of so much importance, that I could wish they might be repeated, and varied, in such a manner as thoroughly to establish the facts relative to the subject in question. For my part, I would most readily undertake the investigation of the matter; but being employed in another pursuit (the continuation of my Experiments upon Heat), and, besides this, much of my time being taken up by the duties of my military employment, I have not leisure, at present, for such an undertaking.

Perhaps it may be proved by future experiment, that the matter of light is a constituent part of what is called pure or dephlogisticated air; if so, may we not venture to conclude with M. SCHEELÉ, that the *light*, as well as the heat, produced by flame, and in general all burning bodies, arises *solely* from the decomposition of this air, and not from the phlogiston or inflammable principle of the body which is burnt? There are many phænomena which would seem to justify this opinion.

But to proceed in the account of my experiments.—The operation of inverting the globes under water, and placing them in the jars, and of displacing and replacing them upon removing the air produced, being attended with some inconveniences, I had recourse to another method of disposing of the apparatus, much more simple and more convenient. The globes being filled were laid upon a small piece of deal board, with their necks inclined at an angle of about 20° above the plane of the horizon, and supported in this position by a perpendicular fork of wood, fixed to the end of the board, as represented by the figure. (See Tab.VI.) The part of the board, upon which the under part of the globe reposed, was hollowed a little, to prevent the globe from rolling; or, what I found

more

more safe and convenient, a small ring, or hoop, of soft wood, was nailed down upon the board in that part.

By this arrangement the jars were spared, and the end of the neck of the globe being easy to be come at, by introducing a wire, or, what I commonly made use of in preference, a small glass tube, into the globe, the air hanging attached to the silk can at all times be separated from it; which is often necessary, in order to determine with greater precision the quantity of air furnished in any given time.

The air produced naturally rises to that part of the globe which is uppermost, where it collects in a body, driving out an equal volume of water; which, to prevent its running about, may be collected, by placing a proper vessel under the mouth, or end of the neck of the globe, to receive it.

The method of removing the air from the globe is too well known to require a description. I would however observe, that in doing it care should be taken, that the water in which the globe is immersed be quite clean, and of the same kind with that in the globe, otherwise that which enters the globe, to replace the air removed, might derange the experiment.

Having provided myself with a number of globes of different sizes, all fitted with boards or stands to support them, in the manner above described, I proceeded in the course of my experiments as follows.

Finding that raw silk, exposed in water to the action of light, causes the water to yield pure air in so great abundance, I was desirous of finding out whether this arose from any peculiar quality possessed by the silk; or whether other bodies might not be made to produce the same effect: to this end, having provided 6 globes, each about $4\frac{1}{2}$ inches in diameter, and having filled them with fresh spring-water, I introduced into
them

them the following substances, and exposed them all, at the same time, to the action of the sun's rays.

In the globe N° 1. I put 15 grains of sheep's wool,

N° 2. — 15 grains of Eider down,

N° 3. — 15 grains of the fine fur of a Russian hare,

N° 4. — 15 grains of cotton wool,

N° 5. — 15 grains of lint, or the ravelings of fine linen,

N° 6. — 15 grains of human hair; these substances being all well washed, and being thoroughly freed of air, by being wet before they were put into the globes.

The results of these experiments were as follows.

Experiment N° 6.

The globe N° 1. which contained the sheep's wool, did not begin to furnish air in any considerable quantity till the third day of its being exposed to the action of the sun's rays; and several days of cloudy weather intervening, I did not remove the air till the eighth day, when I collected $1\frac{1}{4}$ cubic inch, which, proved with nitrous air, gave $1a + 3n = 1,28$, or 272 degrees.

The wool at no time furnished more than one-third part of the air, which an equal quantity of silk would have furnished under the same circumstances.

The water was very faintly tinged of a greenish hue.

Experiment N° 7.

The water in the globe N° 2. with the Eider down, began almost immediately to furnish air, and continued to yield it during the whole time of the experiment, nearly in as large quantities

quantities as the water with silk had done in the former experiments, and nearly of the same quality. $1\frac{1}{4}$ cubic inches of this air, furnished the eighth day from the beginning of the experiment, or the third of sunshine, proved with nitrous air gave $1a + 3n = 1,34$, or 266 degrees of purity.

The water was faintly tinged of a greenish, yellowish cast, and the Eider down, when examined attentively, appeared to be covered with a greenish slime.

Experiment N° 8.

The globe N° 3. with the hare's fur (which was white) furnished more air than the sheep's wool, but not so much as the Eider down. After four days of sunshine, I collected 2 cubic inches of this air, which, proved with nitrous air, gave $1a + 3n = 1,44$, or 256.

The water had acquired a very faint yellowish hue; but it did not appear to have lost much of its transparency, or to be disposed to deposit any sediment.

The air produced in this experiment made its appearance in a different manner from that furnished in those preceding it, the air-bubbles which appeared upon the surface of the fur being at considerable distances from each other, and growing to an uncommon size before they detached themselves to rise to the surface of the water.

Experiment N° 9.

The globe N° 4. with cotton wool furnished a considerable quantity of air which appeared to be better than that furnished by any of the five other globes. Proved with nitrous air, it turned out $1a + 3n = 1,07$, or 293; and, what was particular, the water did not appear to have altered its colour in the least, or to have lost any thing of its transparency.

Experiment N° 10.

The globe N° 5. with ravelings of linen, was very tardy in furnishing air, and produced but a small quantity; at the end of a fortnight, however, I collected about 2 cubic inches, which, proved with nitrous air, gave $1a + 3n = 1,51$, or 249.

The air appeared to have very little disposition to fix itself to the surface of this substance. It was very seldom that there were air-bubbles enough attracted to it to cause it to rise to the surface of the water, and the few bubbles which occasionally made their appearance very soon disappeared upon the diminution of the light and heat of the sun. In short, it appeared, that there is but a very feeble attraction between this substance and the particles of air, at least when they are dissolved in water. Whether this arises from the superior affinity of the substance to water, or not, I will not pretend to decide; but it appears to be probable, as there is so strong an attraction between water and linen, or flax, which is apparent from the avidity with which a piece of dry linen drinks up that fluid, and becomes wet, even to a considerable distance, when one end of it only is placed in it.

You will recollect that I here consider the separation of the air from water as a simple operation; and that I do not take into the account the purification, or rather the generation, of this air. Though there is great reason to conclude, that these two operations are very nearly connected; yet, to simplify my inquiries, I shall, in the first place, consider the appearances as they presented themselves to my senses. It will be easy afterwards to draw any conclusions from the results of the experiments, which a careful examination and comparison of the various phænomena will justify.

Expe-

Experiment N° 11.

The globe N° 6. with human hair, furnished still less air than that with ravelings of linen in the last mentioned experiment; but, notwithstanding the smallness of the quantity, it was considerably superior in quality to atmospheric air, for, proved with nitrous air, it gave $1a + 2n = 1,45$, or 155; whereas common air, proved at the time, gave $1a + 1n = 1,08$, or 92.

Experiment N° 12.

To ascertain the relative goodness of the air furnished by the water in these experiments, and of that produced by exposing fresh healthy vegetables in water to the action of the sun's light, according to the method of Dr. INGEN-HOUSZ, I collected a small quantity of air from a stem of a pea plant, which had four healthy leaves upon it, and found it to be much inferior to that furnished in the experiments with silk, and the various other substances I made use of. Proved with nitrous air, it gave $1a + 2n = 1,05$, or 195.

An intire plant of housewort, of a moderate size, exposed in 12 ounces of water 7 hours, to the action of the sun's rays, at a time when the weather was remarkably fine, and very hot, furnished about $\frac{3}{4}$ of a cubic inch of air, which was so much worse than common air, that 1 measure of it, with 1 measure of nitrous air, occupied 1,36 measures; or it was $1a + 1n = 1,36$, or 64. But I lay no kind of stress upon the result of this experiment, as it is more than probable, that the badness of the air arose from the roots of the plants; for from the leaves alone I have frequently since obtained air, which appeared to be considerably better than common air.

From the leaves of the peach-tree I obtained an air which, proved with nitrous air, gave $1a + 2n = 1,32$, or 168; but I did not think it necessary to multiply these experiments, particularly as Dr. INGEN-HOUSZ and Mr. SENNEBIER have given us the results of so many of theirs upon the same subject, of the accuracy of which there is no room left to doubt. I shall therefore content myself with referring to the results of their experiments.

With a view to determining, with greater precision, the quantity and the quality of the air produced by a given quantity of water and silk, exposed for a given time to the action of the sun's rays, I made the following experiment.

Experiment N^o 13.

A globe of fine, clear, white glass, about $8\frac{3}{10}$ inches in diameter, and containing 296 cubic inches, being filled with fresh spring water and 30 grains of raw silk, was exposed in my window three days, *viz.* the 12th, 13th, and 14th of May last, these days being for the most part cold and cloudy, with short intervals of sunshine. Air produced $9\frac{1}{2}$ cubic inches; quality $1a + 3n = 1,61$, or 239.

May 15. This air being removed, and its place supplied with fresh water, the globe exposed in the sun this day from nine o'clock in the morning till five o'clock in the afternoon, the weather being very fine, yielded $8\frac{4}{10}$ cubic inches of air, which, proved with nitrous air, gave $1a + 4n = 1,74$, or 326. The heat of the water in the globe, during the experiment, was from 70° to 98° F. The water had now lost considerably of its transparency, and had assumed a light greenish hue.

May 16. The air furnished yesterday being removed, the globe furnished this day, during six hours of sunshine, 9 cubic inches

inches of air, which, proved with nitrous air, gave $1a + 4n = 1,44$, or 356.

May 17. The globe furnished this day, during $3\frac{1}{2}$ hours of sunshine, 6 cubic inches of air, of a very eminent quality; for, proved with nitrous air, it gave $1a + 4n = 1,35$, or 365.

May 18. This day cold and cloudy; not more than $1\frac{1}{2}$ hours sunshine; air produced $\frac{3}{4}$ of a cubic inch; quality $1a + 4n = 1,56$, or 344.

May 19. The globe appearing now to be quite exhausted of air, shewing no signs of furnishing any additional quantity, though exposed to the action of a very bright sun, I removed the globe from the window, and placed it by the side of a German stove, where it was kept warm to 100° F. from 10 o'clock in the morning till 5 o'clock in the afternoon. By this means I obtained $\frac{1}{4}$ of a cubic inch of air, which, proved with nitrous air, gave $1a + 4n = 1,74$, or 326.

Not being able to obtain any more air from the globe, I now put an end to the experiment.

The quantities and qualities of the airs furnished upon the different days were as follows:

	Quantity.	Quality.
Upon the 12th, 13th, and 14th of May	$9\frac{1}{2}$ cubic inches	$1a + 3n = 1,61$, or 239
15th	$8\frac{4}{100}$	$1a + 4n = 1,74$, or 326
16th	9	$1a + 4n = 1,44$, or 356
17th	6	$1a + 4n = 1,35$, or 365
18th	$\frac{3}{4}$	$1a + 4n = 1,56$, or 344
19th	$\frac{1}{4}$	$1a + 4n = 1,74$, or 326
Total quantity	$33\frac{96}{100}$	Mean quality $1a + 4n = 1,84$, or 316

As in this experiment the air furnished each day was removed at night, and the place it occupied in the globe supplied with fresh

fresh water, I was desirous of seeing what variation it would occasion in the result of the experiment, if, instead of removing the air from time to time, I suffered it to remain in the globe till the end of the experiment: to this end I made

Experiment N° 14.

In which the globe being filled with fresh water, and the filk used in the last experiment (being first well washed), the whole was exposed four days to the action of the sun's rays, the weather being remarkably fine, and very hot. Upon removing the air produced, I found it amounted to $30\frac{1}{10}$ cubic inches; and its quality, proved with nitrous air, was $1a + 3n = 1,02$, or 298,

I should have continued the experiment for some days longer, as the globe did not appear to be exhausted; but the quantity of air already collected in the globe was so great that it became very difficult to remove it, without running the risk of losing a part of it, or of letting the air of the atmosphere enter the globe, either of which would of course have spoiled the experiment. For safety therefore, and that I might not by an accident lose the trouble I had already had with it, I put an end to the experiment at the end of the fourth day.

The water had lost of its transparency, and had acquired a greenish cast, as in the last experiment; and in both these experiments I observed, that a considerable quantity of whitish yellowish earth was precipitated by the water, which, falling to the bottom of the globe, attached itself to the glass in such a manner that it was with difficulty that it could be removed. These were general appearances, and took place in all cases, in a greater or less degree, where a considerable quantity of pure air was separated from water by the influence of light.

Experiment N^o 15.

The silk made use of in the last experiment having been frequently used in the foregoing experiments, I was desirous of seeing the effect of making use of fresh silk; and also of varying the proportion between the quantity of silk, the quantity of water, and the size of the globe; accordingly, at 6 o'clock P.M. upon the 13th of June, I filled a small globe, about 3 inches in diameter, or (to ascertain its size more exactly) which contained just 20 cubic inches, with fresh spring water, and 17 grains of raw silk, wound in a single thread, which had never been put into water, or otherwise used, since it came out of the hands of the silk-winder.

At the end of four days, *viz.* the 14th, 15th, 16th, and 17th of June, this globe had only furnished $\frac{1}{4}$ of a cubic inch of air, which, proved with nitrous air, gave $1a + 1n = 1,32$, or 68; consequently was much worse than common air.

Upon the 18th, it began to produce good air, and during six hours of sunshine it furnished $1\frac{1}{10}$ cubic inches, which, proved with nitrous air, gave $1a + 3n = 1,15$, or 285.

The two following days (*viz.* the 19th and 20th of June) it furnished $1\frac{2}{10}$ cubic inch of air, which, proved with nitrous air, gave $1a + 3n = 1,37$, or 263; after which it totally ceased to yield air, though exposed for several days in the sun's rays.

Total quantity of air produced $2\frac{6}{10}$ cubic inches; mean quality $1a + 3n = 1,46$, or 254.

By this experiment it appears, that raw silk, when used for the first time, does not immediately dispose the water to yield pure air; on the contrary, that it phlogisticates the air yielded
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by water to a very considerable degree; and this I afterwards found to be the case with several other substances.

Though the quality, at a medium, of the air furnished in this experiment was not quite so good as that furnished in the two experiments last mentioned (*viz.* N° 13. and N° 14.), yet its quantity, in proportion to the quantity of water made use of, was greater than in either of them: it amounted to something more than *one-eighth* of the volume of the water.

Of all the substances I had hitherto made use of in these experiments, raw silk had furnished the greatest quantity of pure air; or, to express myself more properly, had caused the water to furnish the greatest quantity; but it appeared to me very probable, that some other body might be found, that possessed this property in a still greater degree than silk. Turning this matter in my mind, it occurred to me, to make the experiment with the silky, or rather cotton-like, substance produced by a certain species of the Poplar-tree, *Populus nigra*, very common in this country, and which, I believe, grows in England. I recollected that examining it some time before, with a different view (that of seeing if it might not be made use of with advantage, as a substitute for Eider down), and endeavouring to render it very dry, by exposing it in a china plate over a chafing-dish of hot embers, when it had acquired a certain degree of heat small parcels of it quitted the plate of their own accord, and mounted up to the top of the room.

This convinced me of the time not only of its extreme fineness, but also of the strong attraction which subsists between it and the particles of air; and it now occurred to me, that these qualities not only render it peculiarly proper as a substitute of Eider down, for confining heat, but likewise are
properties

properties of all others the most necessary to its supplying the place of silk in the production of air, by exposing it in water to the action of the sun's rays. I therefore lost no time in making the following experiments.

Experiment N° 16.

The great globe (contents 296 cubic inches) being filled with fresh spring water, and 120 grains of poplar cotton, upon the evening of the 9th of June, and being the next day, the 10th of June, exposed to the sun about four hours, upon the morning of the 11th the air produced was removed, and its quantity was found to be $1\frac{1}{4}$ cubic inch. Its quality was very bad, viz. $1a + 1n = 1,65$, or 35 degrees only better than thoroughly phlogisticated air.

Upon the 11th, 12th, and 13th, 1 cubic inch of air only was produced, and this appeared to be as bad as possible; for, proved with nitrous air, it gave $1a + 1n = 2$, or 0.

Upon the 14th a few air-bubbles only were furnished; but, notwithstanding these unfavourable appearances, I still continued the experiment, and my patience was amply rewarded; for the next day, the 15th, the sun being very powerful, and the weather very hot, the water changing suddenly to a greenish colour, began all at once to give good air in great abundance. In the course of the day $10\frac{4}{100}$ cubic inches were produced, which, proved with nitrous air, gave $1a + 3n = 1,43$, or 257.

June 16th, a very warm clear day. The globe exposed in the sun, from 8 o'clock in the morning till 5 o'clock in the afternoon, furnished $14\frac{3}{100}$ cubic inches of air, which, proved with nitrous air, gave $1a + 3n = 1,34$, or 266.

June 17th, cloudy, with intervals of sunshine. The globe with about 4 hours sun gave $7\frac{24}{100}$ cubic inches of air, of a very eminent quality, viz. $1a + 4n = 1,40$, or 360.

The water having by degrees lost its transparency, and having acquired a deep green colour, it broke up this day, and deposited a green sediment; after which it recovered its transparency, and became almost colourless. It continued, notwithstanding, to furnish air in considerable quantities.

June 18th, being exposed in the sun's rays from 8 o'clock in the morning till 2 o'clock in the afternoon (when the heavens became overcast), the globe yielded $6\frac{27}{100}$ cubic inches of air, which, proved with nitrous air, gave $1a + 4n = 1,44$, or 356.

June the 19th and 20th. These two days the globe furnished no more than $3\frac{13}{100}$ cubic inches of air, which, proved with nitrous air, gave $1a + 3n = 1,06$, or 294; after which it ceased totally to furnish air, and the colour of the water changed to a dead yellowish cast, and the cotton assumed the same hue.

The following are the quantities and qualities of the different parcels of air furnished in the course of this experiment.

	Quantity.	Quality.
Upon the 10th of June	$1\frac{3}{4}$ cubic inches	$1a + 1n = 1,65$, or 35
11th, 12th, and 13th	1	$1a + 1n = 2$, or 0
14th	0	—
15th	$10\frac{42}{100}$	$1a + 3n = 1,43$, or 257
16th	$14\frac{34}{100}$	$1a + 3n = 1,34$, or 266
17th	$7\frac{34}{100}$	$1a + 4n = 1,40$, or 360
18th	$6\frac{27}{100}$	$1a + 4n = 1,44$, or 356
19th and 20th	$3\frac{13}{100}$	$1a + 3n = 1,06$, or 294
Total quantity	$44\frac{1}{4}$ Mean quality	$1a + 3n = 1,23$, or 277

This

This experiment was repeated, and with nearly the same result; the total quantity of air produced being $41\frac{1}{2}$ cubic inches, and its quality, at a medium, $1a + 3n = 1,26$, or 274.

To ascertain the relative fineness of this poplar cotton, and the thread of raw silk as spun by the worm, in order to make an estimate of the surface of the former, I examined them both at the same time under an excellent microscope, when the diameter of the cotton, that is to say, of a single thread or fibre of it, appeared to be not more than half as great as the diameter of the silk, consequently its diameter was not more than $\frac{1}{3648}$ part of an inch; for I have shewn, in a former letter, that the diameter of a thread of silk, as spun by the worm, is only $\frac{1}{1824}$ of an inch.

The specific gravity of the cotton I found to be very nearly equal to that of water, consequently it is to that of silk as 1000 to 1734; its surface, therefore, is to the surface of an equal weight of raw silk in the compound proportion of 2 to 1, and of 1734 to 1000; that is to say, as 3468 to 1000.

Now, as the surface of 30 grains of raw silk amounts to 476 square inches, the surface of 30 grains of poplar cotton must amount to 1651 square inches, which gives 55 square inches of surface for each grain in weight; consequently the surface of the cotton made use of in the foregoing experiment (N° 16.) did not amount to less than 6600 square inches (for 120 grains, the weight of the cotton, multiplied by 55, gives 6600); an enormous surface indeed for a body, whose *solid contents* did not amount to quite half a cubic inch.

From hence it appears evidently, that the quantities of air furnished by water, in the experiments with raw silk, and with poplar cotton, were neither in proportion to the quantities of these substances made use of, nor to the quantities of their

surfaces. It appears likewise, from the two last experiments, that the air which is furnished in the beginning of the experiment, or when the water is first exposed to the action of the sun's rays, is neither so good, nor in so great abundance, as afterwards, at a more advanced period; and that it totally ceases to be produced after a certain time.

To ascertain, with greater precision, the qualities of the air furnished at different periods of the experiment, or rather the period when the water begins to give good air; and also to determine the relative quantities and qualities of the airs produced in the experiments with raw silk, and in those with poplar cotton, I made the following experiments.

Experiment N° 17.

A globe, about $4\frac{1}{2}$ inches in diameter, containing just 46 cubic inches, being filled in the evening with fresh spring water, and 30 grains of raw silk which had been previously washed thoroughly to free it of air and the remains of former experiments, and being exposed the next day in my window, the weather being cold and cloudy, with not more than 1 hour of sunshine, $\frac{1}{4}$ of a cubic inch of air was produced, which, proved with nitrous air, gave $1a + 2n = 1,86$, or 114.

The two following days, the weather being clear and moderately warm, $3\frac{1}{4}$ cubic inches of air were produced, which, proved with nitrous air, gave $1a + 3n = 1,14$, or 296.

Experiment N° 18.

The globe having been again filled with fresh spring water, and the same silk which had served in the last experiment, after 2 nights, and 1 day of about 4 hours sun, it had furnished $1\frac{1}{2}$ cubic inch of air, whose quality was $1a + 2n = 1,13$, or 197.

The

The two following days, the weather being very fine, it furnished $3\frac{8}{10}$ cubic inches of air, which, proved with nitrous air, gave $1a + 4n = 1,58$, or 342.

Experiment N^o 19.

The globe being again filled with fresh water, and the same silk well washed, and being exposed 2 days in the sun, it gave $2\frac{2}{10}$ cubic inches of air, which, proved with nitrous air, gave $1a + 3n = 1,67$, or 233.

Experiment N^o 20.

A like globe, with fresh water, and an equal quantity of poplar cotton which had been used in former experiments, being exposed at the same time, gave $2\frac{5\frac{3}{8}}{10}$ cubic inches of air, whose quality was $1a + 3n = 1,20$, or 280.

Experiment N^o 21.

A small globe, contents 20 cubic inches, with 17 grains of raw silk, exposed at the same time, gave 1 cubic inch of air, which turned out $1a + 3n = 1,37$, or 263.

Experiment N^o 22.

A large globe, containing 296 cubic inches, being filled with fresh water and a small quantity of *conserva rivularis*, and exposed at the same time with the three globes above-mentioned, gave $1\frac{1}{2}$ cubic inch of air, which, proved with nitrous air, gave $1a + 2n = 1,76$, or 124.

The water in this experiment was changed to a brown colour, owing, as I conceived, to the too great heat the *conserva* acquired in the sun.

These experiments were made between the 2d and the 5th of July.

Experiment N° 23.

Surprised at the smallness of the quantity, and the inferior quality, of the air produced in the last experiment, I was induced to repeat it; accordingly, the globe being again filled with water, and a quantity of fresh *conferva rivularis* (a small handful), and being exposed to the action of the sun's rays during 3 fine days, $13\frac{34}{100}$ cubic inches of air were produced, which, proved with nitrous air, gave $1a + 3n = 1,54$, or 246.

At the end of the experiment the water appeared to be very faintly tinged of a greenish cast.

The two following experiments were made upon the 20th and 21st of August.

Experiment N° 24.

A globe, about $4\frac{1}{2}$ inches in diameter (contents 46 cubic inches) being filled with fresh spring water, and 30 grains of raw silk which had been used in many preceding experiments, and being exposed to the action of the sun's rays two days, in all about 8 hours of sunshine, the weather being cloudy great part of the time, $1\frac{6}{100}$ cubic inch of air was produced, which, proved with nitrous air, gave $1a + 3n = 1,96$, or 204.

Experiment N° 25.

At the same time an equal globe, containing fresh spring water, and about 15 grains of poplar cotton (which had likewise been used in former experiments) produced $1\frac{28}{100}$ cubic inch of air, which, proved with nitrous air, gave $1a + 3n = 1,40$, or 260.

The

The water in both these experiments had acquired a faint greenish cast; but the colour of that with the cotton was rather the deepest.

Upon examining this water under a microscope, I found it contained a great number of animalcules, exceedingly small, and of nearly a round figure. That with the silk contained the same kind of animalcules likewise, but not in so great abundance. I never failed to find them in every case in which the water used in an experiment had acquired a greenish hue; and from their presence alone, I think it more than probable, that the colour of the water, *in the first instance*, arose in all cases. I have spent a great deal of time in observing them, and have made many experiments upon their production; but as I have not yet been able to satisfy my own mind, with respect to the part they act in the operation of purifying the air in water, I shall not add to the length of this letter by giving an account of my enquiries and observations respecting them.

I was yet by no means satisfied with respect to the part which the silk and other bodies, exposed in water in the foregoing experiments, acted in the purifying or dephlogificating of the air produced.

Dr. PRIESTLEY has long since discovered, that many animal and vegetable substances putrefying, or rather dissolving, in water, in the sun, cause the water to yield large quantities of dephlogificated air; but I could hardly conceive, that the small quantity of silk which was used in my experiments, and which had been constantly in water for more than three months, and had so often been washed, and even boiled in water, should yet retain a power of communicating any thing to the large quantities of fresh water in which it was successively placed; at least *any thing* in sufficient quantities to impregnate

nate these bodies of water, and to cause them to yield the great abundance of air which they produced.

It was still more difficult to account for the purification of the air in the experiments with wool and fur, and human hair; especially, as in some of these experiments the water had not sensibly changed colour, nor did it appear to have lost any thing of its transparency. It is true, in these cases, the quantities of air produced were very small; but yet its quality was better than that of common air, and considerably superior to that of the air existing in the water, previous to its being exposed to the action of the sun's light. In short, it was dephlogisticated in the experiment; but the *manner* in which this was done is very difficult to ascertain.

With a view to throwing some new light upon this intricate subject, I made the following experiments.

Experiment N° 26.

Concluding that if silk and other bodies, used in the foregoing experiments, actually did not contribute any thing, considered as chymical substances, in the process of the production of pure air yielded by water; but if, on the contrary, they acted merely as a mechanical aid in the *separation* of the air from the water, by affording a convenient surface for the air to attach itself to; in this case, any other body, having a large surface, and attracting air in water, might be made use of instead of silk in the experiment, and pure air would be furnished, though the body so made use of should be totally incapable of communicating *any thing whatever* to the water.

To ascertain this fact, washing the great globe (containing 296 cubic inches) perfectly clean, and filling it with fresh spring water, I introduced into it a quantity of the fine flexible
1
thread

thread of glafs, commonly called *spun glafs*, fuch as is ufed for making brushcs for cleaning jewels, and for making a kind of artificial feather frequently fold by the Jew pedlars. This spun glafs is no other than common glafs drawn out, when hot, into an exceeding fine thread; which thread, in confequence of its extreme finenefs, retains its flexibility after it has grown cold.

I made choice of this fubftance not only on account of its great furface, but alfo on account of the ftrong attraction which is known to fubfift between glafs and air, and the impoffibili of its communicating any thing to the water.

The refult of the experiment was, that the globe being expofed in the fun, air-bubbles began almoft immediately to make their appearance upon the furface of the spun glafs, and in 4 hours $\frac{77}{100}$ of a cubic inch of air was collected, which, proved with nitrous air, gave $1a + 1n = 1,12$, or 88; after which, not a fingle air-bubble more was produced, though the globe was expofed a whole week in the window, during which time there were feveral very warm, fine, funfhiny days.

This experiment fhews evidently, that fomething more is wanting to the production of pure air by water, expofed in the fun, than merely a furface to which the air diffolved in the water can attach itfelf, in order to its making its efcape.

The air furnifhed in this experiment was doubtlefs merely that with which the water iffuing from the earth was overcharged, and which would have made its efcape from the water, had the water, inftead of being expofed with the spun glafs in the fun, been fimplly left for fome time expofed to the free air of the atmofphere.

It appears, that this air, naturally exifting in fpring water, inftead of being dephlogifticated, is fomething worfe than

common air; and this agrees with the observations of Dr. PRIESTLEY, and seems to justify his opinion with respect to the cause of the fertility of lands washed by waters issuing from the earth.

If the above experiment shews that something is wanted to be mixed with water, in order to enable it to yield pure air, when exposed to the action of the sun's light, the following shew, that this *something*, whatever it may be, is frequently to be found in the water itself, in its natural state.

Experiment N° 27.

A large jar of clear white glass, containing 455 cubic inches, being washed very clean, was filled with fresh spring water, and inverted in a glass basin of the same, and placed in the middle of the garden of the Elector's Palace, where it was left exposed to the weather 28 days.

At the same time another like jar was filled with water, taken from a pond in the garden, in which many aquatic plants were growing, and was exposed in the same place, and during the same period. This water had a very faint greenish cast. The pond from which it was taken is fed by a large river (the Ifar), which runs by the town.

The second day after these waters had been exposed in the sun, I observed, that a small quantity of air had collected itself at the upper part of each of the jars.

The third, fourth, and fifth days, the pond water furnished air in pretty large quantities; and it went on to yield it without intermission, when the sun shone upon it, till the fourteenth day, when it seemed to be nearly exhausted. I continued the experiment, however, till the twenty-eighth day, though during the

the last fortnight the quantity of air in the jar did not appear to be sensibly increased.

The spring water, during the first five or six days, furnished very little air; and it was not till the fourteenth day that it began to yield it in any considerable quantities. From this time it went on to furnish it, though but very slowly, till about the twenty-second day, when it ceased, appearing to be quite exhausted.

Upon the twenty-eighth day I removed the airs from the jars, when I found their quantities and qualities to be as follows:

	Quantity.	Quality.
Air furnished by the spring water	14 cubic inches	$1a + 2n = 1,62$, or 138
by the pond water	$31\frac{1}{2}$ -	$1a + 3n = 1,48$, or 252

Neither the colour of the spring water, nor that of the pond water, appeared to be sensibly changed; but both the one and the other of these waters had deposited a considerable quantity of earth, which was found adhering to the surfaces of the glass basons in which the jars were inverted.

As these basons were rather deep, and as they were very thick in glass, and consequently not very transparent, their bottoms, where the sediment of the water was collected, were, in a great measure, obscured or deprived of the direct rays of the sun. Suspecting that this circumstance might have had some effect, so as to have hindered the water from furnishing so much air as otherwise it might have yielded, to satisfy myself respecting this matter I repeated the experiment, disposing the apparatus in such a manner, that the sediment of the water, which attached itself to the bottom of the vessel in which the jar was inverted, had the advantage of being perfectly illuminated.

Experiment N^o 28.

In a large cylindrical jar, of very fine transparent glass, 10 inches in diameter, and 12 inches high, filled with spring water, I inverted a conical glass jar, $9\frac{3}{4}$ inches in diameter at the bottom, and containing 344 cubic inches, filled with the same water; and exposed the whole 21 days, in a window fronting the south.

The quantity of air produced amounted to 40 cubic inches; and its quality, proved by the test of nitrous air, gave $1a + 3n = 1,87$, or $21\frac{3}{4}$.

The water in this experiment furnished very little air till the seventh day; but after that time, having assumed a faint greenish cast, and a fine greenish slimy sediment (the *green matter* of Dr. PRIESTLEY) beginning to be formed upon the bottom of the jar, it began to yield air in abundance, and continued to furnish it in pretty large quantities till about the eighteenth day, when it appeared to be exhausted.

Why the water should turn green in this experiment, and not in the last, I know not; unless it was in consequence of the large surface of water in the cylindrical jar, which was exposed to the air in this experiment; or in consequence of the sun's shining directly upon the bottom of the vessel where the sediment was formed.

In the former experiment the basin in which the jar was inverted was but just big enough to admit the jar; and as the jar was cylindrical, the surface of the water exposed to the atmosphere, in the basin, was but very small; and the basin being very thick, and formed of glass which, though of the white kind, was of an inferior quality, and very imperfectly transparent, as I have already observed, the bottom of the basin, where

where the sediment was formed, was but very imperfectly illuminated.

I intended to have repeated these experiments with variations, and to have made several others which I had projected, and which I thought might have thrown some further light upon this wonderful process of the production of the pure air yielded by water; but a series of unfavourable weather putting a stop to my enquiries, and my time having been much taken up since with other avocations, I have hitherto been prevented from putting my designs in execution; and the season proper for these experiments is now so far advanced, that I do not think it will be in my power to recommence them till the next year. In the meantime, to fulfil my promise to you, I send you this account of the progress I have already made in these researches; and, when I shall find leisure to pursue the matter further, I shall not fail to acquaint you with the result of my enquiries.

I have the honour to be, &c.

P O S T S C R I P T.

SINCE writing the above, an interval of fine weather, and a moment of leisure, have given me an opportunity of making a few more experiments, of which I have thought it right to give you a short account.

And I must begin by acquainting you, that having never been thoroughly satisfied with respect to the origin of the dephlogificated air produced upon exposing fresh vegetables in water to the action of the sun's rays, according to the method of Dr. INGEN-HOUZ, my doubts, with respect to the opinion generally entertained of its being *elaborated* in the vessels of the
plant,

plant, instead of being removed, were rather confirmed by the result of the experiments of which I have given an account in the foregoing letter; and however disposed I was to adopt the beautiful theory of the purification of the atmosphere by the vegetable kingdom, I was not willing to admit a fact which has been brought in support of it, till it should appear to me to have been demonstrated by the most decisive experiments.

That the fresh leaves of certain vegetables, exposed in water to the action of the sun's rays, cause a certain quantity of pure air to be produced, is a fact which has been put beyond all doubt; but it does not appear to me to be by any means so clearly proved, that this air is "*elaborated* in the plant by the "powers of vegetation;"—"phlogificated or fixed air being "first absorbed or imbibed by the plant as food, and the dephlogificated air being rejected as an excrement:" for, besides that many other substances, and in which no elaboration, or circulation, can possibly be suspected to take place, cause the water in which they are exposed to the action of light to yield dephlogificated air as well as plants, and even in much greater quantities, and of a more eminent quality, the circumstances of the leaves of a vegetable, which, accustomed to grow in air, are separated from its stem, and confined in water, are so unnatural, that I cannot conceive, that they can perform the same functions in such different situations.

Among many facts which have been brought in support of the received opinion of the *elaboration* of the air in the vessels of the plants in the experiments in question, there is one upon which great stress has been laid, which, I think, requires further examination.

The fresh healthy leaves of vegetables, separated from the plant, and exposed in water to the action of the sun's rays,

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appear,

appear, by all the experiments which have hitherto been made, to furnish air *only for a short time*; after a day or two, the leaves changing colour, cease to yield air: and this has been conceived to arise from the powers of vegetation being destroyed; or, in other words, the death of the plant; and from hence it has been inferred, with some degree of plausibility, not only that the leaves actually retained their vegetative powers for some time after they were separated from their stock, but that it was in consequence of the exertion of these powers, that the air, yielded in the experiment, was produced.

But I have found, that though the leaves, exposed in water to the action of light, actually do cease to furnish air, after a certain time, yet that they *regain* this power after a short interval, when they furnish (or rather cause the water to furnish) more and better air than at first, which can hardly be accounted for upon the supposition that the air is *elaborated* in the vessels of the plant.

Experiment N^o 29.

A globe, containing 46 cubic inches, filled with fresh spring water and two peach leaves, was exposed in the window to the action of the sun's rays, 10 days successively (the weather being in general fine), when the following appearances took place.

The 1st and 2d day, a certain quantity of air was produced, about as much as in former like experiments. The 3d day very little was produced; and the 4th day none at all, the globe to all appearance being quite exhausted. Continuing the experiment, however, upon the 5th day, the water having acquired a faint greenish hue, air was again produced pretty plentifully, *making its appearance upon the surface of the leaves in the form of air-bubbles,*

bubbles, as at the beginning of the experiment; at the end of the 6th day the air was removed, and it was found to amount to $\frac{5.4}{100}$ of a cubic inch, its quality being 232 degrees, or $1a + 3n = 1,68$.

Upon the 7th day $\frac{2}{100}$ of a cubic inch of air was produced of 297 degrees, or $1a + 3n = 1,03$; and

During the 8th, 9th, and 10th days, $1\frac{1}{2}$ cubic inch of air, of 307 degrees (or $1a + 4n = 1,93$), was furnished; after which an end was put to the experiment.

Total quantity of air produced $3\frac{1.9}{100}$ cubic inches; mean quality 291 degrees, or $1a + 3n = 1,09$.

Finding that leaves which were dead, or in which all the powers of vegetation were evidently destroyed, continued notwithstanding to separate air from water, and that in so great abundance, I was desirous of seeing the effect of exposing fresh healthy leaves in water which I knew to be previously saturated with, and disposed to yield dephlogificated air. I conceived, that if the plants exposed in water actually imbibed fixed or phlogificated air as food, and, after digesting it, "discharged" the dephlogificated air as an excrement;" in that case, as there is no instance of any plant, or animal, being able to nourish itself with its own excrement, the leaves exposed in water saturated with dephlogificated air, instead of imbibing and elaborating it, would immediately die.

The experiments which I made to ascertain this fact, and which, without any comment, I shall submit to your consideration, were as follows.

Experiment N° 30.

Having provided a quantity of water, which, by being exposed with a few green leaves in the sun, had acquired a greenish cast, and which I found was disposed to yield dephlogificated

gified air in great abundance, I filled a globe, containing 46 cubic inches, with this water, and putting to it two healthy peach leaves, exposed the globe in the sun upon the 7th of September, from 11 o'clock in the morning till 2 o'clock in the afternoon (3 hours), when $\frac{7}{10}$ of a cubic inch of air was produced, which, proved with nitrous air, gave $1a + 3n = 1,52$, or 248 degrees.

A like globe, with fresh spring water and two peach leaves, exposed at the same time, furnished only $\frac{1}{5}$ of a cubic inch of air, which, on account of the smallness of its quantity, I did not submit to the test of nitrous air.

Experiment N^o 31.

September 8. Very fine clear weather, but rather cold for the season. Three equal globes, A, B, and C, containing each 46 cubic inches, were filled as follows, and exposed in the sun from 9 o'clock in the morning till half an hour past 4 in the afternoon, when they were found to have produced air as under mentioned.

The globe A, filled with water, which, by being previously exposed in the sun for several days, with potatoes cut in thin slices, had turned green, furnished $\frac{2}{10}$ of a cubic inch of air of 299 degrees, or $1a + 3n = 1,01$. N. B. This water, before it was put into the globe, was strained through two thicknesses of very fine Irish linen.

The globe B, filled with the same green potatoe water (strained as before) to which were added four middling-sized peach leaves, furnished $2\frac{1}{2}$ cubic inches of air of 320 degrees, or $1a + 4n = 1,80$.

The globe C, filled with fresh spring water, with four peach leaves, furnished $\frac{5}{10}$ of a cubic inch of air of 151 degrees, or which, proved with nitrous air, gave $1a + 2n = 1,49$.

To ascertain the quantities and qualities of the airs remaining in the different waters used in this experiment, putting the globes separately over a chafing-dish of live coals, and making the water boil, taking care to hold the globe in such an inclined position as that the air separated from the water might be collected in the upper part of the globe, the airs produced were as follows.

	Quantity.	Quality.
By the green water in the globe A,	$\frac{85}{100}$ of a cubic inch	280 degrees
By the green water in the globe B,	$\frac{21}{100}$ — —	241
By the spring water in the globe C,	$\frac{11}{100}$ — —	68

The waters in these experiments were made to boil but for a moment; otherwise, it is probable, more air might have been separated from them.

Finding that fresh leaves, exposed to the action of the sun's rays, in water which had already turned green, caused pure air to be separated from the water in so great abundance, I repeated the experiment, only, instead of leaves, I now made use of a small quantity of *conferva rivularis*; when I had nearly the same result as with the leaves.

To ascertain the relative quantities and qualities of the airs yielded by the green water, when exposed with fresh leaves, and when exposed with raw silk; and also to ascertain, at the same time, how long leaves, exposed in green water, retain their power of separating air from it, I made,

Experiment N° 32.

Two equal globes, A and B (containing 46 cubic inches), the former (A) filled with green potatoe water, strained through linen, and four peach leaves; the latter (B) filled with the same potatoe water, strained in like manner, and 17 grains of
raw

raw filk, were exposed from Sunday noon, September 10th, till Monday evening, the weather being cold, with many flying clouds, in all about 6 or 7 hours sun.

The airs produced were as follows.

	Quantity.	Quality.
By the globe A, with green water and 4 peach leaves	$2\frac{7}{16}$ cubic inches	292 deg.
By the globe B, with green water and 17 grs. of raw filk	$2\frac{7}{16}$ - -	307

Another globe (C), filled with green water *alone*, was exposed at the same time; but it was broken by an accident before the experiment was completed.

The two globes (A and B) with their contents, being again exposed from Tuesday noon till Thursday evening, yielded air as follows.

	Quantity,	Quality.
The globe A, with the peach leaves	$4\frac{47}{168}$ cubic inches	344 degrees
The globe B, with raw filk -	$4\frac{3}{16}$ - -	350

N. B. The weather on Tuesday and Wednesday was cold, with very little sunshine; but Thursday was a very fine warm day, when the greatest part of the air was produced. This air was removed and proved on Friday morning the 15th September.

Perhaps all the appearances above described might be satisfactorily accounted for, by supposing the air produced in the different experiments to have been generated in the mass of water by the *green matter*; and that the leaves, the filk, &c. did no more than *assist it in making its escape*, by affording it a convenient surface to which it could attach itself, in order to its collecting itself together, and taking upon itself its elastic form.

The phenomena might likewise be accounted for by supposing the *green matter* to be a vegetable substance, agreeable to the hypothesis of Dr. PRIESTLEY, and that attaching itself to the surfaces of the bodies exposed in the water, as a plant is attached to its soil, it grows; and, in consequence of the exertion of its vegetative powers, the air yielded in the experiment is produced.

I should most readily have adopted this opinion, had not a most careful and attentive examination of the green water, under a most excellent microscope, at the time when it appeared to be most disposed to yield pure air in abundance, convinced me, that, *at that period*, it contains nothing that can possibly be supposed to be of a vegetable nature. The colouring matter of the water is evidently of an animal nature, being nothing more than the assemblage of an infinite number of very small, active, oval-formed animalcules, without any thing resembling *tremella*, or that kind of *green matter*, or water moss, which forms upon the bottom and sides of the vessel when this water is suffered to remain in it for a considerable time, and into which Dr. INGEN-HOUZS supposes the animalcules above-mentioned to be actually transformed.

But having finished the account of my experiments, I shall finish my letter, not daring to venture conjectures upon a subject so intricate in itself, and which is yet so new, and upon which the ablest philosophers of the age seem to be so much divided in opinion.



